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REJOINDER TO PROFESSOR SHOREY

The chief point in my criticism¹ of Professor Shorey was his failure to see in the recent study of education—some of which has been experimental—anything that is at all valuable. In his specific attack upon the “transfer” experiments, I believed that he could have very effectively called attention to the unwarranted interpretations and extensions to which these experiments have been subjected, without at the same time blinding his public to their really valuable outcomes. I particularly resented his listing me as an opponent of the doctrine of formal discipline when the very instance that he cited² from me as “gravely alleged” against the doctrine was explicitly brought forward (as the context³ clearly shows) to illustrate the possibilities of transfer. The first article that I published on formal discipline⁴ was directed against what I believed to be dangerous and unwarranted interpretations of the earlier experiments, and involved in essence the notion of transfer upon the conscious level which has been thoroughly sanctioned by practically all of the investigations that permit the operation of a conscious factor. Against the notion of transfer upon an automatic level I have, indeed, protested strongly, and here again the experiments in general bear me out.

It is possible, of course, that Professor Shorey will object to my contention that the valuable features in the doctrine of formal discipline may be retained and the harmful features eliminated by the “simple” formulation of the doctrine in terms of “concepts of method” or “ideals of procedure.” He may wish to retain the implication of transfer on the automatic level, and he may assert that my rejection of this notion is really a rejection of the doctrine as a whole. In that case, he should have taken pains to define his own idea of discipline—an issue which, I regret to say, he explicitly dodges. In the “Case for the Classics,” he admits that the term “discipline” is “perhaps equivocal or question-begging,” and asserts that it is not to be “authoritatively defined.” I should not conclude that his own idea of discipline lacks clearness; I certainly should not list him among those unfortunate persons who “have no clear ideas on the subject without or in advance of experimentation”; it would be quite unkind to suggest that his own admitted inability to give a clear definition of the term should impel him to seek the aid of experimentation, which he admits to be valuable in such cases. The fact remains,

¹ *School Review*, May, 1912, pp. 343-46. Professor Shorey's reply is published in the *School Review*, June, 1912, pp. 417-21.

² “The Case for the Classics,” *School Review*, November, 1910, p. 608, note.

³ *The Educative Process*, pp. 210 ff. The illustration in question is prefaced by the statement that certain explanations of transfer put forward by O'Shea and Thorndike leave something unaccounted for. After describing the “gravely alleged” experience and after citing two other cases, I definitely assert that something very important and very powerful may be carried over from one field to another. On p. 216 the essential validity of the doctrine of formal discipline is definitely asserted.

⁴ *School and Home Education*, October, 1904.

however, that he did dodge a definition; consequently I am unable to say whether my formulation of the disputed doctrine tallies in any way with his own idea. If, on comparison, he finds that it does, I have no doubt that the promised apology will be forthcoming; in any case, it is a matter of minor consequence.

It is not of minor consequence, however, that Professor Shorey should impute to the experimental method the misconstructions that may have been placed upon its results. It is legitimate criticism to prove that an experiment simplifies conditions in a deceptive and falsifying way, but it is incumbent upon the critic to do something more than simply make the assertion. He must, if I understand aright the canons of scientific criticism, show clearly where the conditions of the experiment belie the actual conditions to which the experimental results are generalized. Legitimate simplification of conditions involves the elimination of irrelevant factors; it is the critic's business to show that the factors eliminated are not irrelevant, if he would cast suspicion upon the experiment from this point of view. Professor Shorey has here a fair field for his recognized critical ability, but he fails to exploit it. All that I can find is the purely dogmatic assertion that "inserting needles into holes, estimating areas, drawing with the hand hidden behind a screen, etc., etc., are all falsifying simplifications of the infinitely complex problem to the solution of which they may or may not lead in years to come."¹ We should welcome a careful study by Professor Shorey of, say, Ruger's experiments on the psychology of efficiency,² with a clear statement of how far we may extend Ruger's conclusion that the transfer of training is not found on the automatic level, but that ideals of procedure are wonderfully effective agencies in such transfer.

Again it is not a matter of minor consequence that Professor Shorey should dismiss as too "simple to be recognized" the terms "concept of method" and "ideal of procedure." Again, if I understand aright the canons of criticism, one cannot crush a theory by the mere assertion that it is simple. Simplicity at this point if anywhere is a virtue and not a fault. But Professor Shorey can do the world a good turn if he proves that a theory is too simple to offer an adequate control over the processes involved. This, I may suggest, is another point where his criticism would be welcomed. Let him take, for example, the excellent "rules for transfer" formulated upon the basis of the experimental results by Professor Colvin³ and show either that they will be positively detrimental in the practice of teaching or that they offer nothing that could have been inferred from the disciplinists' conception in its older form.

It would, of course, be futile to waste words with Professor Shorey as to the value of the experiments on the psychology of thought. These have

¹ "The Case for the Classics," p. 607, note.

² *Archives of Psychology*, June, 1910.

³ *The Learning Process*, pp. 242 ff.

but slight educational significance at present, and I have already asserted that their results are meager enough. If, however, Professor Shorey can find among his favorite authorities an adequate recognition of the importance of the *Aufgabe* and the *Bewusstseinslage*, or a convincing statement of the relation of kinaesthesia to meaning, the citation would be interesting.

Professor Shorey's reference to Professor Thorndike's article is obviously superficial as well as quite oblivious to the crying need in elementary education of efficiency-scales. Professor Thorndike has here pioneered his way into a field that promises the largest practical results. I can testify from my own experience to the value of his handwriting standards in simplifying the work of supervision, and I have no doubt that the composition-scale will be equally serviceable. The terminology that Professor Shorey objects to is only a tentative attempt to label something that has not yet been analyzed. Metaphor in a case of this sort is not to be confused with the metaphorical designation of psychological processes for which fairly unequivocal terms are already available.

In conclusion, I may assure Professor Shorey that my use of the term "code of honor" and my employment of other "unfortunate" phraseology should be considered as instances of "rhetorical exaggerations," comparable

believe in every way to his own use of the following phrases with reference to my profession: "Recent writers upon education do not *scruple* to tell the public that science has spoken"; "the humbler service of protestants who decline to be *gulled*"; "for the protection of a *gullible* public"; "they must not *turn one face* to the public and *another to us*." (The italics are mine.)

W. C. BAGLEY

THE UNIVERSITY OF ILLINOIS